REVIEWS

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Julia, A Life in Mathematics. By Constance Reid. Mathematical Association of America, Washington, D.C., 1997, xi + 125 pp., $29.

Reviewed by Lenore Blum

In July 1996, the Association for Women in Mathematics (AWM) held the Julia Robinson Celebration of Women in Mathematics at the Mathematical Sciences Research Institute in Berkeley as part of AWM's year long 25th Anniversary celebration. In August 1997, another conference to celebrate women mathematicians in number theory and analysis was held at Berkeley. And there are more.

Thirty years ago, such celebratory identification would have sounded the professional death knell. Lest you had any doubt, if you were a female graduate student at the time you may very well have been told in "jest" by one of your professors: "There've only been two women mathematicians. One wasn't a woman and one wasn't a mathematician." And if your name was identifiably female, you would not have been admitted to the graduate program in mathematics at Princeton. It was not until 1968 that women were allowed.

How did we get from there to here? While the 1960's might be characterized as a time of naiveté, denial, and lying low, the early 1970's became a time of consciousness raising. But then, quickly, the community of women mathematicians moved into a proactive stance and developed constructive programs to increase the participation of women in mathematics. These yielded positive results, respect, and self-confidence that set the stage for achievement, recognition, and celebration. Honoring the "two women mathematicians," AWM inaugurated the first of its annual Emmy Noether Lectures in 1980 and, in 1985, its Sonya Kovalevsky High School Days **[7]**.

We have learned that it is within a broad and historical context that one understands and values the lives and achievements of mathematicians, women and men.

Julia, A Life in Mathematics **[20]**, eminently engaging and accessible, evokes such a perspective, implicitly and explicitly. A compilation of four previously published articles (starting with a beautifully formatted "Autobiography of Julia Robinson" by Constance Reid, then articles by Lisl Gaal, Martin Davis, and Yuri Matijaseivich), it proves to be a great deal more than the sum of its parts.

The four articles reinforce as well as complement each other, sometimes relating the same story from different vantage points. Indeed, the evolution of the solution to Hilbert's tenth problem, as seen through the eyes of the key protagonists, is a wonderful, exciting, and very human story of mathematical progress with all its ups and downs.

Here is a telescopic view.[1] Hilbert's tenth problem (HTP), as posed in 1900 at the International Congress of Mathematicians in Paris, is to find an effective method to determine if a given Diophantine equation is solvable in integers.

During the 1930's logicians made precise the informal notion of "effective method," and raised the possibility (of showing) that no such method exists. A strategy for demonstrating such a negative result would be to show that a known "undecidable" problem could be restated in terms of the solvability of certain Diophantine equations. Such would be the case if all recursively enumerable sets of natural numbers (nonnegative integers) were Diophantine. Think of a *recursively enumerable* (*r.e.*) set as a set listable by a computer program. These computer programs are in turn listable, so each r.e. set A can be associated with a (Gödel) number, which we denote by $[A]$.

A set A of natural numbers is *Diophantine* (or *existentially definable*) if there is a polynomial $P_A(x, y_1, \dots, y_n)$ with integer coefficients with the property that a belongs to A if and only if $P_A(a, y_1, \dots, y_n) = 0$ is solvable in integers, i.e.,

$$(*) \qquad a \in A \text{ if and only if } \big(\exists y_1 \dots \exists y_n \in \mathbf{Z}\big(P_A(a, y_1, \dots, y_n) = 0\big)\big).$$

Using standard arguments, we can stipulate solvability in the natural numbers $\mathbf{N}$ instead of in $\mathbf{Z}$.[2] Diophantine relations are similarly defined.

It is easy to see that Diophantine sets are recursively enumerable. On the other hand, the Diophantineness of all r.e. sets seemed a bit preposterous since this would imply the existence of a *universal* d, N, and Diophantine equation $P(u, x, y_1, \dots, y_N) = 0$ of degree d in the variables $y_1, \dots, y_N$ that define (as above) every r.e. set A (after replacing u with $[A]$). Hence, for example, the set of prime numbers would be so defined by P.[3] It would also follow that *every* Diophantine equation $D(a_1, \dots, a_m, y_1, \dots, y_n) = 0$ with *parameters* $a_1, \dots, a_m$ would be equivalent (in terms of solvability) to one with the same parameters and of degree d in N unknowns (the universal d and N).[4]

Julia started working on HTP after receiving her Ph.D. in 1948. Alfred Tarski, her thesis advisor, had suggested to Raphael Robinson, her husband, that the set of powers of 2 was not Diophantine.[5] Julia tried to show it was. This she was unable to do, but instead showed **[22]** that

(1) the relation $y = x^z$ is existentially definable in terms of any relation of "exponential growth."

Here x, y, and z are natural numbers.

[1] For technical expositions see **[9]**, **[10]**, and **[15]**.

[2] One uses the facts that (by Lagrange) natural numbers can be written as sums of 4 squares (of integers) and that integers can be written as differences of natural numbers. Of course, in each case, the polynomials defining A differ.

[3] Even more, letting $p = [(\text{primes})]$ and $f(x, y_1, \dots, y_N) = x(1 - P^2(p, x, y_1, \dots, y_N))$, we see that the primes are precisely the positive output value of the polynomial f (over all natural number input values).

[4] Here, codings by Gödel numbers are used **[15**, Chapter 4].

[5] Mathematical ideas are often communicated and developed within informal social settings, the "lunch" being one of the most fruitful such venues. In those days, women were not allowed in the main dining room for lunch at the Men's Faculty Club at Berkeley, so Julia often heard about mathematical ideas via Raphael.

A relation $J(u, v)$ is of *exponential growth* if it "grows faster than a polynomial but not too terribly fast." That is, let $J = \{(a, b) \in \mathbf{N}^2 | J(a, b) \text{ holds}\}$. Then

1. for each $k > 0$ there exists $(a, b) \in J$ with $b > a^k$ and
2. for each $(a, b) \in J, b < a^a$.

For $y = x^z$ to be *existentially definable* in terms of J means: for all $a, b, c \in \mathbf{N}$,

$$(**) \quad b = a^c \text{ if and only if } (\exists w_1 \dots \exists w_n \in \mathbf{N})(Q(a, b, c, w_1, \dots, w_n, J))$$

where $Q(x, y, z, w_1, \dots, w_n, J)$ is a formula built up from the variables $x, y, z, w_1, \dots, w_n$; the mathematical symbols $0, 1, +, \times, =$; logical connectives & and $\vee$; and the relation J.

Martin Davis also had been thinking about HTP since his graduate student days at Princeton. He was able to show **[8]** that every recursively enumerable set could be defined in a way that seemed almost Diophantine.[6] Meeting in 1950 at the first post-war International Congress at Harvard, Julia and Martin compared their distinct attacks on HTP.[7] Building on their combined approaches, they later published a paper with Hilary Putnam **[11]** showing that

(2) every recursively enumerable set of natural numbers is exponential Diophantine.

Exponential Diophantine sets A are defined as in $(*)$ but now the polynomial $P_A(x, y_1, \dots, y_n)$ may have exponents that are variables.

Thus by (1), the negative resolution to HTP, a metamathematical result, would follow from showing a purely number theoretic result, namely,

(J.R.) there is some Diophantine relation of exponential growth,

as hypothesized in Julia's 1952 paper and later dubbed "J.R." by Martin.

Thus one need only exhibit a Diophantine equation $B(u, v, x_1, \dots, x_m) = 0$ with the property that the set of *parameter* pairs

$$J = \{(a, b) | (\exists x_1 \dots \exists x_m \in \mathbf{N}(B(a, b, x_1, \dots, x_m) = 0)\}$$

is a relation of exponential growth.

Many mathematicians thought this approach was misguided and, as far as I know, virtually no one else pursued this direction. In *Mathematical Reviews*, Georg Kreisel **[13]** was particularly sharp:

These results **[11]** are superficially related to Hilbert's tenth problem. ... The proof of the authors' results, though very elegant, does not use recondite facts in the theory of numbers nor in the theory of r.e. sets, and so it is likely that the present result is not closely connected with Hilbert's tenth problem. Also it is not altogether plausible that all (ordinary) Diophantine problems are uniformly reducible to those in a fixed number of variables or fixed degree, which would be the case if all r.e. sets were Diophantine.

In the 1960's when he would give talks on HTP, Martin would emphasize the important consequences of a proof or disproof of J.R. To the inevitable query on

[6] In particular, Martin showed that every r.e. set A can be defined as in $(*)$ if one just replaces the second quantifier $\exists y_2$ by the *bounded quantifier* $\forall y_2 \leq y_1$. This representation is known as the *Davis normal form*.

[7] In the "Autobiography," it incorrectly states that both Julia and Martin gave ten-minute talks at the Congress on their work on HTP. Julia did; Martin had already reported on his results at an ASL meeting the previous winter.

how did he think matters would turn out, he had an ever-ready reply: "I think that Julia Robinson's hypothesis is true, and it will be proved by a clever young Russian."

Yuri Matijaseivich was a sophomore at Leningrad State University in 1965 when he started thinking about HTP. Dissuaded at first from looking at the work of the "Americans" ("So far [they] have not succeeded, so their approach is most likely inadequate"), it was not until 1968 that he sought out their papers in translation. Immediately he started spending all his free time looking for a Diophantine relation of exponential growth, but got nowhere. Embarrassed to continue working on a famous problem with no success, he steeled himself from thinking more about it, even refusing to look up Julia's new paper when it came out in 1969. Luckily ("there must be a god or goddess of mathematics"), Yuri was obliged to review it for the Soviet counterpart of *Mathematical Reviews*.

In this paper **[24]**, Julia attempted to prove J.R. by considering solutions to a special form of Pell's equations $x^2 - (a^2 - 1)y^2 = 1$, $a > 0$, a form she had already used in her 1952 paper. Reinspired, Yuri started considering similar equations whose solutions are the Fibonacci numbers. Shortly after New Year's day 1970, he finished constructing a Diophantine equation $B(u, v, x_1, \dots, x_m) = 0$ existentially defining the relation $v = F_{2n}$, where F_k is the kth Fibonacci number **[14]**.

In early February, Grigorii Tseitin presented a talk on the work in Novosibirsk. John McCarthy was present and sent his rough notes to Julia when he returned to the States. Immediately, Julia reconstructed the proof and a few days later wrote Yuri:

> ... now I know it is true, it is beautiful, it is wonderful. If you really are 22, I am especially pleased to think that when I first made the conjecture you were a baby and I just had to wait for you to grow up.

When Martin met Yuri that summer at the International Congress in Nice, he was finally able to tell him that "he had been predicting his appearance for some time."

In a real sense, Yuri was the graduate student Julia never had (more on this later). They became great friends and collaborators. In his contribution to *Julia*, there is a delightful account of the mathematical trials and tribulations of their joint work that reduced the universal N (for solvability in the natural numbers) from 200 to 13 **[16]** (later, Yuri was able to further reduce N to 9)[8] and glimpses into life before e-mail and before the end of the cold war.

The work on HTP, probably the most familiar of Julia's mathematical contributions, is part of a body of work that lies in the interface between logic and algebra, and might be categorized as applied logic. This area was the theme of Julia's 1980 American Mathematical Society Colloquium talks, "Between logic and arithmetic."[9] It had already been an underlying theme of her thesis **[21]**. There, using theorems of Hasse on quadratic forms, Julia had shown that the integers are arithmetically definable in terms of the rationals and hence, as a consequence of the landmark case of the integers, the arithmetic theory of the rationals is not

[8] It follows that 36 unknowns suffice for universality and undecidability of Diophantine equations over the integers. Zhi-Wei Sun has apparently reduced this number to 11.

[9] There she pointed out that it was still an open question, as it remains today, whether there is an effective method to decide if a given Diophantine equation has a rational solution. This is equivalent to asking if there is an effective method to decide whether a given homogeneous Diophantine equation has nontrivial integer solutions.

decidable. (A set A is *arithmetically definable* if in $(*)$ some of the existential quantifiers are allowed to be universal. A discussion of Julia's dissertation is in Lisl Gaal's contribution.)

My first introduction to Julia's work was in the 1960's when I was a graduate student at M.I.T. I had been fascinated by emerging work of Jim Ax and Simon Kochen **[2]**, **[3]**, and **[4]** solving problems about p-adics using (non-standard methods of) model theory. (I had been able to get some related results about differential fields **[5]**.) Julia's beautifully written paper, "The decision problem for fields," was a constant reference **[23]**. It provided an overview of what was known at the time, presented a wealth of interesting ideas, and discussed several open problems, some of which have subsequently been resolved.[10] It's a paper I distributed to my logic class at Berkeley several years later and, over the years, have had occasion to refer to from time to time. A faded purple mimeographed copy is with me as I write this in Hong Kong. Luckily, the American Mathematical Society has published *The Collected Works of Julia Robinson* **[12]** where one can find all her papers and also a fine memoir by Sol Feferman.

"The Autobiography of Julia Robinson," written by her sister Constance Reid during the last month before Julia died of leukemia in 1985, conveys a real sense of Julia's life and spirit. It is a delight to read.

Julia's ability to work independently throughout her life—and in her own way—was certainly shaped by events early on: the death of her mother when she was a toddler; a bout with scarlet fever when she was nine, followed by rheumatic fever, which prevented her from attending school for two years. Her fascination with numbers was apparent at an early age:

> One of my earliest memories is of arranging pebbles in the shadow of a giant saguaro, squinting because the sun was so bright. I think that I have always had a basic liking for the natural numbers. To me they are the one real thing.

The "Autobiography" embraces Julia's rightful importance in the history of women in mathematics; yet at the same time it appears to discount, or perhaps ignore, limitations placed on her mathematical career merely because she was a woman. Julia was part of the Berkeley logic community from her student days until her death; yet there is no questioning of that community's role in her lack of official stature at Berkeley for much of her professional life.

That said, the "Autobiography" in *Julia* is considerably more revealing than its original incarnation (in **[19]** and later in **[1]**). Included now on almost every page are previously unpublished photographs and memorabilia found among Julia's things. It's a wonderful treat for mathematicians, for mathematics teachers and students, and even for nonmathematicians.

Probably the most fascinating document for me is a handwritten draft note, apparently written to Paul Cohen, exhibiting a feistiness not usually part of Julia's public persona. After he showed the independence of the continuum hypothesis in 1963 (thus solving Hilbert's first problem), Paul turned his attention to the

[10] Lou van den Dries has shown that the ring of algebraic integers is decidable **[25]**; Alex Prestel and J. Schmid showed the same for the ring of real algebraic integers **[18]**. I am grateful to Lou for pointing me to these and other references, particularly the survey **[17]**.

Riemann Hypothesis (RH), perhaps with the thought that it too might be independent. In the first paragraph of her note, Julia chides:

> Paul, you should know that if RH is undecidable from PA [Peano Arithmetic] then it is true! and for heaven's sake there is only one true number theory! ~~It's my religion~~ that's why it is so exciting to prove anything at all about it.

Anyone wishing to get a *hint* of Julia's candid views on logic might want to carefully examine the rest of this intriguing note (on p. 49), both for what is left in and what is crossed out.

At the end of the book there is a one-page curriculum vitae. Read without reference to the times, the quantum leap in status in 1976, and subsequent recognition, it appears totally bizarre. Read in historical context, it speaks volumes:

> Born, December 8, 1919, St. Louis, Missouri;
> Graduated, San Diego HS (1936); San Diego State (1936–39);
> AB (1940), MA (1941), PhD (1948), UC Berkeley;
> Junior Mathematician, RAND Corporation (1949–50); ONR (1951–52);
> Lecturer, UC Berkeley (Spring 1960);
> Lecturer, UC Berkeley (Fall 1962);
> Lecturer, UC Berkeley (1963–64);
> Lecturer, UC Berkeley (Spring, Fall 1966);
> Lecturer, UC Berkeley (Fall 1967);
> Lecturer, UC Berkeley (1969–70);
> Lecturer, UC Berkeley (Spring 1975);
> Election, National Academy of Sciences (1976);
> Professor, UC Berkeley (1976–1985);
> Distinguished Alumnus, San Diego State (1978);
> Election, AAAS (1978);
> Vice-President, AMS (1978–79);
> Honorary Degree, Smith College (1979);
> Colloquium Lecturer AMS (1980);
> Noether Lecturer, AWM (1982);
> MacArthur Foundation Prize (1983);
> President AMS, (1983–84);
> Election, AAAS (the other one) (1985);
> Died, July 30, 1985, Oakland, California.

In the "Autobiography," Julia relays events surrounding her election to the National Academy of Sciences:

> When the University press office received the news, someone there called the mathematics department to find out just who Julia Robinson was. "Why, that's Professor Robinson's wife." "Well," replied the caller, "Professor Robinson's wife has just been elected to the National Academy of Sciences." Up to that time I had not been an official member of the University's mathematics faculty, although from time to time I had taught a class at the request of the department chairman. In fairness to the University, I should explain that because of my health, even after the heart operation, I would not have been able to carry a full-time teaching load. As soon as I was elected to the Academy, however, the University offered me a full professorship with the duty of teaching one-fourth time—which I accepted.

I remember when I first came to Berkeley in 1968 I was quite surprised to find that Julia was not a member of the Berkeley faculty; I like many others had always assumed she was. In response to my queries, I was given vague reasons alluding to her health, nepotism rules, and her ranking on some linearly ordered list of logicians. As to her health, an *offer* of a position with reduced teaching would have

been appropriate, as her statement suggests. I actually looked up the nepotism rule and found that it explicitly allowed for exceptions to be made on the recommendation of the department chair.

What about Julia's ranking in logic? Recall, the 1960's were glory days for pure mathematics and abstraction. Applied mathematics was viewed somewhat disdainfully, perhaps as not deep or hard enough. Applied logic was similarly slighted by logicians obsessed with set theory and higher recursion theory. To a great extent Julia was affected by these prejudices.[11]

Logic missed out. Mathematics missed out. Berkeley missed out. Had Julia been a regular member of the Berkeley faculty she would surely have supervised some bright mathematics graduate students. Certainly, she would have suggested a try at the hypothesis J.R. There is a good chance Hilbert's tenth problem would have been solved at Berkeley. The pieces were all there. Who knows what other gems would have been uncovered? And perhaps logic would not have become so isolated from the rest of mathematics.

Julia's election to the National Academy of Sciences in 1976 opened the way for institutions and associations to award her position and recognition long overdue. And, clearly these events helped Julia move into the limelight. By accepting the presidency of the American Mathematical Society (AMS), Julia knew she would be thrust into the role of public person, and she rose to the occasion. But also, on a more personal level, this public recognition, I believe, helped Julia acknowledge the significance of her own contributions to mathematics.

As I wrote in [**6**], in this regard, I feel privileged to have been able to witness a side of Julia that I think many other mathematicians rarely had a chance to see. One particular encounter stands out vividly.

Sometime after Julia became president of the AMS, we arranged to meet for lunch with Nancy Kreinberg of the Lawrence Hall of Science to discuss ways to encourage girls and women in mathematics. I remember arriving at the Women's Faculty Club at Berkeley a bit early, just as the noontime bells were beginning to ring. Julia was already there. Sitting in one of those high-backed Victorian chairs, wearing a bright floral dress, she looked quite majestic. I sensed a special occasion. As I came up to greet her, she broke into one of her broad impish grins. "Guess what?" she said excitedly. "I have just been awarded the MacArthur prize—for my part in solving Hilbert's tenth problem!"

To celebrate, we ordered a bottle of champagne. I can't remember now the details of our conversation. But I do remember clearly the warmth of three women thoroughly enjoying each other's company, sharing feelings of excitement, triumph, and plans for the future. And, by the end of the meal, after several rounds of toasts, we had very nearly finished the bottle of champagne. Later, when I mentioned this lunch to a friend in the Berkeley Mathematics Department, he protested, "But don't you know? Julia Robinson never drinks!"

I appreciate that Julia included me in her joy. It's a way in which I will always remember her.

[11] Similar prejudices were to affect the course taken by the then-emerging field of complexity theory, now central to computer science and a source of important mathematical challenges (e.g., does $P = NP$?). Developed initially in the 1960's by researchers originally trained in logic and mathematics, the field took root and flourished in the more hospitable environments offered by newly formed computer science departments.

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Postscript. A preliminary version of this review was circulated to mutual friends and colleagues. Their comments add perspective both to *Julia* and to my review. With regard to the isolation of logic, Sol Feferman proposes another explanation:

I don't think "applied logic" was looked down on by logicians in the 1960's, at least not logic applied to algebra via model theory. And I think the isolation of logic from mathematics is a long-term phenomenon that has little to do with its applications or non-applications to mathematics, and more to do with the nature of its notions and methods, which are not for the most part contiguous with mainstream mathematics. That's a great pity, because, notwithstanding that separation, there is so much of both foundational and mathematical interest that has come out of the work in logic in the last 50 years that ought to be better known and appreciated.

Martin Davis offers affectionate insight into "the full human being Julia was:"

Julia's sensibility was not career oriented in a way hard for people nowadays to fathom. This was partly true because she had internalized the social definition of a woman's role, only beginning to think about

creative mathematics as something she might do after a miscarriage and very much on Raphael's prompting. But it was also the modest style of both of them. After Yuri's wonderful result, she insisted that the solution of HTP be credited entirely to Yuri. Yuri and she had this incredible dance after he had reduced their $N = 13$ universal equation to $N = 9$. He refused to publish unless she signed on as co-author, she refused to sign on because she'd contributed nothing new. Very refreshing!

As you know, she had intended to devote her retiring AMS presidential address to Raphael's work. She was convinced that he was the stronger mathematician. She was little attracted to the idea of a regular position, and told me that she had accepted the part-time professional position because it would be good for women in mathematics, not because she was especially eager for it. She told me that it was a privilege that she could do mathematics when she wished, and drop it to work for Adlai Stevenson (a cousin) when that seemed more important.

She was modest but not at all diffident. This came out in working with her, but also, for example, in the way she dealt with Soviet mathematicians regarding anti-Semitism in the Soviet Union.

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Would-be Worlds. By John L. Casti. John Wiley & Sons, New York, 1996, 242 pp., $24.95.

Reviewed by **Daniel P. Maki**

Simulation is seductive! It attracts the innocent mathematician/scientist with the lure of doing something that seems to be forbidden to those traditionalists who still hope to solve all important real problems by analytical means. In *Would-be Worlds* (WBW), John Casti has made a great case for the notion that we should be seduced by simulation, and, in fact, large scale digital simulation is the only method with any hope of handling the really interesting societal problems that remain to be solved. If you are at all interested in big, complicated problems, such as traffic flow, the evolution of galaxies, the stock market, and global politics, then WBW is a great place to learn about the virtues and power of simulation. The examples are well chosen and convincing, and the discussion is stimulating. However, there is much more in WBW than the siren call of simulation. In fact, this reviewer found the other material to be even more interesting than the discussion and examples about simulation. We need to talk about this other material to explain why simulation is so important.

Mathematical models are the bridges that link mathematics as an abstract art to mathematics as a powerful tool. Mathematical modeling is a hot topic these days, one whose time has (once again) finally arrived. Modeling shows up often in the NCTM Standards for Mathematics in Grades K–12, there is now an international competition in modeling (with about 400 teams entered in 1997), and the National Science Foundation has funded several large curriculum projects that stress mathematical modeling and interdisciplinary applications. Nevertheless, there are still many college mathematics instructors who would be hard pressed to give their students a definition of mathematical modeling. For such faculty members, the cure is at hand: they must read WBW. It provides a helpful overview of modeling in general and mathematical modeling in particular. Even better than the overview of mathematical modeling is the discussion of how models are used and how they are evaluated. That section alone is worth the price of the book.